Emergent Structure: The First Two Centuries of the First Two Eons

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Abstract. Scientific recognition of the existence, evolution, and significance of structure within the cosmos developed slowly. We follow the story here from the earliest times to the first systematic redshift surveys and the "Rubin-Ford effect," emphasizing the period beginning with William Herschel and ending about lunch time on Wednesday. The scientific issues cannot be put in any one linear order, because, for instance, some people were studying clusters of galaxies and measuring the mass of M31 while others still denied the existence of external galaxies.

INTRODUCTION

The images shown at the conference and the ideas presented there and here have been drawn from a very large number of secondary and primary sources. Important general ones include Berendzen (1976), Jaki (1972), Whitney (1971), Hoskin (1997), the introductory chapters of Peebles (1993), Harrison (1981, 1987), Smith (1982), and Martinez et al. (2002). Sources with primarily 20th century content include Bok and Bok (1945), Shapley (1943), Lundmark (1956), the chapters in Sandage et al. (1975) by D. Layzer, E. Holmberg, G.B. Field, C.D. Shane, G. de Vancouleurs, G.O. Abell, and A. Sandage, McVittie (1962), especially the articles by E L. Scott and G.B. van Albada, Neyman et al. (1961), and Trimble (1995, 1996, 1997, 1999). References that can be found in these last and in introductions to other "October" proceedings have often not been repeated here.

THE ANCIENTS TO NEWTON

The cosmologies of the Mesopotamians (one version of which you can find in Genesis) and of the Egyptians had a flat earth with square corners and one or more deities carrying out various tasks. The Egyptian Shu (air), for instance, held Nut (sky) up above Geb (earth), so that the sun (Aten) could sail his day and night boats around the configuration. The "stick man" of the Early harvard slides might be thought of as a modern Shu, though he inhabits a universe that is isotropic on larger scales. The Chinese Pan-ku (who also separated heavens from earth) served a similar function.

Assorted Greeks noticed that the earth is closer to a sphere than a plane and measured its size and the distance to the moon with reasonable precision. They also had a strong predilection for circles, spheres, and circular motion on all scales in the cosmos.

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The merging of Greek philosophy with medieval church doctrine that was largely the work of Thomas Aquinas imposed this spherical symmetry on European thought after about 1260, along with immutability of the heavens, the four terrestrial elements + quintessence, and much else. Indeed many of the early readers of Copernicus were inclined to think that the most important thing he was saying was the primacy of uniform circular motion, whatever is at the center. But, before this, the 12th century universe of Hildegaard of Bingen had a spherical earth, but a pineapple-shaped lucidus ignis outside it. She also placed the fixed stars close to us, hail and lightening further out beyond the moon, followed by the sun and outer planets (as the stem of the pineapple). Thus she had not yet fully accepted immutability of the heavens as requiring hail, lightning, comets, meteors, and guest stars all to fall within the atmosphere. A Chinese model universe from about the same time is spherical and held up by dragons (Needham 1953). It also has an equatorial mount, rather than an ecliptic one like the European armillary spheres of the time, and the equatorial concept may well be a Chinese invention

Then the spheres and circles close in, first with earth at the center a la Ptolemy (and Martin Luther), then, increasingly, with the sun at the center, a la Copernicus.

Next is the question of where to put the stars. Outside the orbit of Saturn, clearly, but how far outside, how many of them, and how far should they extend? Early on, these questions tended to get stirred in with old philosophical considerations of whether voids and infinities were conceivable and hence possible. For some people, they probably still do.

Thomas Digges in 1576 extended his "orbe of starres fixed" to infinity (though not in the drawing) and allowed them to have planets and life. He said that the total assemblage though infinite was somehow vaguely spherical, but that it could have neither edges nor a center. Infinite, multiply inhabited, and more or less spherical universes are also to be found in the writings of Nicolas of Cusa (c. 1450), Giordano Bruno (before 1600!), and William Gilbert (d. 1602). Gilbert explicitly allowed the stars in his infinite distribution to be of intrinsically different size (meaning brightness).

Kepler (d. 1630) considered the possibility of an infinite, fairly uniform distribution of stars, of which the sun would be merely an undistinguished one, but rejected both this and a "stoic" universe (with infinite empty space beyond a single sphere of stars) in favor of a bounded cosmos, with a single spherical shell of stars not far outside the orbit of Saturn. Indeed he knew exactly where to put it by requiring that all the Platonic solids nest neatly within one another.

Supporters of finite vs. infinite distributions of stars co-existed right down to the 20th century. Descartes's 1636 world had an infinite number of vortices, with central stars illuminated by rotational motions (and planets forming in the whirls), and regions of influence that were neither spherical nor circular but look like Voronoi tessellation. Otto von Guericke, who in some sense discovered the vacuum (with the Magdeburg evacuated metal sphere and horses experiment) was perfectly happy to have a vacuum outside his sphere of stars. This was his solution to Olbers' paradox (see Harrison 1987 for more on how this consideration influenced early modern cosmologies). Newton in the 1660s seems to have been a finite "coelum stellatum" man, with some combination of chaos and void outside, but he later worried about the gravitational equivalent of Olbers in an infinite universe and evolved a sort of solution.

WILLIAM HERSCHEL AND THE SHAPE OF THE MILKY WAY

No one who actually looked at the sky would suppose that the distribution of stars around us is spherically symmetric, whether finite or infinite. The first disk galaxy does not, however, appear until the writings of Emmanuel Swedenborg (1734). He envisioned a sort of magnetic dipole structure (analogous to that found for the earth by Gilbert in 1600) and an infinite number of other Milky Ways filling space in a hierarchical arrangement.

Thomas Wright of Durham, who appears in the history paragraphs of many elementary astronomy texts, switched in about 1750 from a uniform ("promiscuous") distribution of stars to a slab, and his edge-on view of the Galaxy, though sun-centered, looks modern for its time, with stars of different intrinsic brightnesses and a reasonable diameter to thickness ratio. But his "face on" view is much more like a Greek central fire universe or an Eye of God. He also packed them hierarchically, again for partly Olbersian reasons.

William Herschel was an active contributor to astronomical thought from the 1760s until very close to his death in 1822. Thus he had plenty of time to change his mind and, eventually, rode all possible horses in the race involving nebulae all resolving into stars vs. some being truly diffuse, a single island universe (Galaxy) vs. other nebulae being other, comparable galaxies, and finite vs. infinite The picture with which he is most often associated dates from 1785 and charts the Milky Way via what he called star gauging. That is, assume all stars are the same real luminosities and figure out their distances from apparent brightness. Then distribute them more or less uniformly in space out to an edge set by the numbers you see in each direction. Inevitably, this puts the solar system in the plane of the Milky Way and very close to the center of a disk that extends a thousand light years in radius and a couple hundred in thickness., with slices in the edges corresponding to the Cygnus rift and such.

The elder Herschel's confidence in all stars being the same brightness was strong enough that he compiled lists of close pairs of unequal apparent brightness and followed their relative separation carefully) in the hopes of measuring parallax. In fact, he found the first few partial orbits of binary stars.

William Huggins settled the "resolvability" issue in 1868 in favor of truly diffuse, gaseous nebulae by finding emission lines in the specta of the Orion nebula and a couple of Herschel's planetaries. Majority opinion then settled in around a picture in which a disk-like galaxy or region of stars (with the sun near the center and filamented edges) had a region of nebulae on either side. The pictures, of course, had to have edges, so you could not be quite sure how far the zone of the nebulae extended!

Simon Newcomb (1878) favored such a picture for many years. He was, as director of the US Naval Observatory, the inevitable first president of the American Astronomical Society (called something else initially). He has had rather bad press within the community, largely for his opposition to astrophysics (meaning the introduction of spectroscopy into astronomy). It is, therefore, perhaps worth recalling that he also asked some very prescient questions at the turn of the previous century (Newcomb 1906). Among these were; (1) what is the size of the universe, and is there a boundary? (2) are the volume and duration finite or infinite? (3) what is the form and extent of the Milky Way, and is it conceivable that our apparent centrality means that we are the victims of some fallacy, like that afflicting Ptolemy? (Way to go, Simon!), and (4) where do the stars of large proper motion come from and go to? He had in mind that they would leave the visible galaxy in only millions of years, and the answer of course eventually involves one in identifying stellar energy sources, recognizing that star formation is a continuous process, and even invoking dark matter.

Some remarkable reconstructions of the Milky Way as it might appear from outside appear in the first decades of the 20th century, Easton (1900) knew that we were supposed to be at the center of the whole system but were clearly not at the center of the distribution of bright star clusters and nebulae. His face-on galaxy has a system of spiral arms whose center has been pushed off to one side in the direction of Cygnus, while we reside in a sparser region at $r = 0$ of a somewhat arbitrary circle. Eddington's edge on galaxy had a central cloud of stars and a ring around it (Eddington 1912).

Harlow Shapley counts as an official culture hero for getting us permanently out of the center of the Milky Way (Shapley 1919). He used pulsating variable stars to estimate distances to the globular clusters, which he recognized were heavily concentrated on one side of the sky, and so put us 20 kpc from the center of the whole Galaxy (and the spiral nebulae inside). Kapteyn, close to death in 1922, held by a much smaller disk, with the sun very near the center, and today is not much remembered, except for this "Kapteyn universe" and his selected areas.

The missing ingredient was, of course, interstellar obscuration (absorption and scattering) by dust. Kapteyn himself had worried about this in the early 1900s. Sanford (1917) and Curtis (1917, 1918) had assembled data suggesting to them that the "zone of avoidance" in the plane of the Milky Way, where few nebulae could be seen, was the result of obscuration in the plane, analogous to the dark lanes to be seen in many edge-on spirals. Indeed the whole Curtis-Shapley debate belongs to this era, and has as one of its subtexts the issue of interstellar dimming of light (as well as the existence of external galaxies and the official topic of the size of the galaxy).

Credit for the discovery of the effects of systematic obscuration goes, however, to Trumpler (1930) and his assemblage of star clusters, whose apparent angular diameters fell less steeply with apparent magnitude than implied by $1/r^2$ alone. Trumpler's (1941) own image of the galaxy at first looks like a wrong-headed attempt to please all parties by putting a Kapteyn universe off on one edge of a Shapley galaxy. What he meant, however, is that there is a small, off-center circle of the disk that we can survey, while the inner disk, center, and far side are largely obscured, except at high latitude. Plaskett's (1939) drawing is very much like the one we all put on the (black, marker, or virtual) boards in classrooms today. The sun is l0 kpc from the center (which was right from Oort's work in the 1930s until Baade shrank the galaxy in 1953, and again from a 1965 IAU resolution in favor of l0 to a successor in 1988, which moved us back in to 8.5 kpc).

Plaskett, Bok and Bok (1945, who drew just the outline of their Milky Way), and we all know that the galaxy is rotating, and that you must be sure to subtract off the rotation velocity if you want to do cosmology with redshifts. The rotation hypothesis was put forward by Lindblad (1924-26), and additional supporting data assembled by Oort (1927), who frequently gets credit for the discovery, though he titled his paper "Observational evidence confirming Lindblad's hypothesis of a rotation for the galactic system."

THE NUMBERS AND NATURE OF NEBULAE

At this point, we have to back up, because some astronomers were already working on this section while others were still arguing about the size of the Milky Way. Lundmark (1956) notes that 21 fuzzy things (including the Hyades and Pleiades, as well as the Orion nebula and Andromeda) were known to the ancients. Some of these must have been lost, since Halley was aware of only six in 1715. LaCaille catalogued 42 southern fuzzies in 1755, and Messier's better known, but less systematic, catalogue has 107. He apparently found their nature of no interest, except that they were non-comets.

William Herschel observed about 1000 nebulae and left drawings of many. Some are (with 20-20th century hindsight) obviously planetaries (which is what he called them); some are irregular HII regions; and others look like edge-on spirals. His telescopes did not, however, reveal any real structure within face-on disks, and he drew no spirals.

Meanwhile, however, Michell (1767) had "pre-discovered" star gauging and estimated the size of the Milky Way (1000 LY or so) . He then calculated that, if some of the more conspicuous nebulae (like M31 and M33) were about the same size, then their brightest individual stars should have V = 13.8. The modern value is 15.6 (but both our distance scales and maximum stellar luminosities have grown a good deal in the interim). Another way to see and say this (though apparently Michell did not) is that if M31 is 4° across in the sky and is rather like the Milky Way, then the distance between us is about 15 times the individual sizes.

The 19th was, altogether, a messy century for nebular astronomy. At midpoint, Rosse (1851) published a drawing of M51 that clearly shows the arms. He described it as a spiral (his word) with stars. Messier had called it a double nebula with no stars, W. Herschel a bright nebula with a halo and a companion, and J. Herschel a divided ring.

This brings us back to the beginning of the 20th century again, with the community still pretty firmly divided on the reality of other galaxies. Agnes Clerke is the person perhaps most often quoted to the effect that no astronomer in full possession of the data (and his faculties) believed in external galaxies. But others, whose names are now also largely forgotten (like Wolfe and Very) felt the same way in various languages.

Meanwhile, Lundmark (1919) had put M31 at 220,000 pc from the novae in it. Curtis's (1917) number was 6 Mpc for several nebulae with novae. And Opik (1922) had estimated a distance and a mass on the reasonable assumption that M/L for the disk ought to be like that of the solar neighborhood. He also had a rotation curve to work with obtained by Pease at Mt, Wilson.

Vesto Melvin Slipher is another of the undersung heroes, who belongs in this section because he took the first nebular spectrogram from which a radial velocity (though not rotation) could be measured (M31 in 1912). He got, rightly, about -300 km|sec. He had been set to the task of getting nebular spectra by Percival Lowell, who expected to find evidence that the spirals were solar systems in formation. Slipher initially concurred,, but by the time he had a dozen examples (most with large positive velocities) he began to suspect that he was seeing something else. He did not attempt a plot of those velocities vs. anything, at least in published form, though others did, by including a distance-dependent K term in their solutions for solar motion. You may, if you wish, regard one or more of these as the "discovery" of Hubble's law. Lemaitre and Wirtz (separately) have perhaps the best claims.

THE REALITY OF LARGER STRUCTURES

We will have to back up again, because many people (including both the Herschels) had noticed that the nebulae were not randomly scattered on the sky outside the zone of avoidance, long before the nature of those nebulae had been settled. In fact, if you happen to know how to distinguish a proper extragalactic nebula from a star cluster, HII region, or planetary nebula, you can discover Virgo, the Local Group, and, marginally, the Leo, CVn, and UMa clusters in Messier's catalog. If, however, you plot all his objects, then the largest concentration is actually globular clusters in the general vicinity of RH = 16-20 hours, declination = 0 to -40 (the galactic bulge), and M1 is, of course, the Crab Nebula, because the expected path of Halley's comet in its 1758 return came nearby.

In the same sort of way, Virgo, Coma, and one or two others can be seen in plots of the objects in J. Herschel's General Catalogue (e.g, Proctor 1869)and Dreyer's 1888 New General Catalogue (e.g. Waters 1894), though there are also concentrations that represent the Large and Small Magellanic Clouds and a certain number of "selected areas" where the cataloguers had looked harder than average. The last person to make such a plot without community agreement about what he was plotting was Charlier (1908, 1922). He was a strong supporter of a fractal or hierarchical universe and so expected to find structure on a range of scales, as indeed he did.

To make further progress, one needs some way of ruling out alternatives to real structure. Differential obscuration lives in Sect. 9. Let's look here at statistical flukes (such as the one that puts the brightest quasar, 3C 273, in the field of the Virgo cluster).

Michell (1767) tackled the statistics problem for binary stars, using the same sort of "one-minus-the-probability-of-the-opposite" that one uses to figure out how many people have to be in a room before two will share a birthday. He got the details somewhat wrong (S. Stigler, pr. comm. 2001), but was completely correct in concluding that (contrary to what Wm. Herschel thought) most pairs of stars in the sky, even of very unequal brightness, are physically associated, as are the star clusters. Polya (1919) redid the calculation, by thinking of putting n points on a sphere and not wanting any one to fall within an angle less than S of another. His answer was

$$P(n/S) = \cos^{2n}(S/2) \approx exp(-nS^2 4).$$

And Bok (1934) did it again, this time having galaxies in mind

As in the stellar case, galaxy pairs were the first structure statistically established, initially by Lundmark (1932), who found about 100 close pairs in NGC and the Shapley-Ames (1932) Catalogue, and then, more carefully, by Holmberg (1937), who was Lundmark's student and who had a very large (55,000) but very non-homogeneous sample of galaxies. He found 695 pairs less than 0.1 ° apart, where chance predicted 42. Holmberg was, in this same paper, the first to estimate galaxy masses from the hypothesis that the pairs were mostly bound by gravitation. His answer was an M/L ratio larger than that found by Hubble for single galaxies, but smaller than that found by Zwicky (1933) for the Coma cluster, but this is part of a different story (one version of which appears in Trimble 1987).

Next after binaries come small groups. Indeed they might well be the same thing, observed to different limiting magnitudes. The Local Group is a binary if you see down only to 10^{10} solar luminosities in V (and a triple a couple of magnitudes below that). The LG was recognized and named by Hubble (1936), who remarked that these (then) six galaxies were grouped much more closely than the general run of extragalactic nebulae. This is a three-dimensional remark. LG members are scattered across much of the sky.

Notice that we have skipped right over the "miracle decade" during which Hubble established, to nearly everyone's satisfaction, that nebulae contain Cepheid variables, leading to distances for them well outside the Milky Way and that these nebulae are moving apart from one another on average. Incidentally, while the distances in the linear velocity-distance-relation were Hubble's own, the velocities came first, from Slipher and, later from Milton Lassell Humason. Again that is part of a slightly different story (Trimble 1995, 1996)

Hubble's Local Group (to which another 30 or so members have been added since 1936) had a radius of about l MLY on his distance scale, and so was too small by a factor 3-4. Thus he concluded that the Milky Way outshone Andromeda by about 10 times, while Andromeda, in turn, outshone not just the other LG members but nearly all the other nebulae by another factor 10. In other words, his distance scale outside the LG was too small by a somewhat larger factor 5-7.

THE PREVALENCE OF CLUSTERING

The recognition that most or all galaxies are grouped and clustered took several decades to pervade the community, though not so long as the time required for all to regard spirals as other galaxies. As late as the 1960s, mainstream astronomers were still sometimes writing or saying that individual galaxies were "the basic building blocks" of the universe (G.O. Abell lectures in 1963, McCrea 1968, Chiu 1968)

Hubble, though he had identified the Local Group, retained a life-long slant in favor of most galaxies being randomly distributed on the sky (e.g. Hubble 1934, 1936) Shapley (1932, 1934), who was an early exponent of "mostly clustered" thought this was because Hubble insisted on using large telescopes, whose fields of view could take in only about one galaxy at a time. Hubble's response was that Shapley would have used large telescopes if he had had access to them. There was indeed little love lost between them (Christianson 1997), at least partly because Hubble had volunteered for active duty as soon as he had defended his 1917 thesis, rather than taking up a position at Mt. Wilson Observatory. By the time he returned from France, Shapley (offered a position at nearly the same time, which he accepted) had been observing for a couple of years and had done some of the things that Hubble later said he had planned for. Shapley moved on to be director of Harvard College Observatory in 1921, while Hubble remained at Mt. Wilson until his death in 1953. Both were, however, members of the IAU Commission (28: Nebulae and Star Clusters) that, at the 1925 General Assembly, emphasized the need for a modern catalogue of nebulae to replace or complement NGC. Shapley's observatory was already at work in the southern hemisphere, and the Shapley-Ames catalog was one of the products. It is characteristic of the men that Hubble belonged only to Comm. 28, while Shapley belonged to 6 others and was president of 27, Variable Stars. Shortly before Hubble's death, he was not even an IAU member, while Shapley was president of

Comm 39 (International Observatories), belonged to 28 and three others, and had family members covering three more .

Specific clusters, like Coma and Virgo, recognizable in 19th century data, had been pointed out by Hinks, Wolf, Wirtz, and others before the settling of the, “island universe” issue and were generally accepted. The next was Hercules (Shapley 1934). Hubble (1934, 1936a) added a couple more, Number 7 was identified by Carpenter (1931) who used it as a point on a density-size plot; Lundmark (1931) picked out a couple, and Tombaugh (1937) found one while he was engaged in another search, which was also successful.

But the time had come again for statistics. Bok (1934), with his reformulation of the “birthday method” said clustering was the norm. He was, of course, at Harvard, where Shapley was still director. Still more, the time had come for a uniform data base. This was provided by Shane and Wirtanen (1954), who covered the northern sky with $6 \times 6^{\circ}$ plates taken at Lick and counted galaxies in small boxes. This yielded a map in which dense regions seem to connect up over wide angles, sometimes in clumps, sometimes in filaments or shells around relative empty regions. And the heaviest possible statistical guns were brought to bear on these counts by Neyman, Scott, and Shane (1953). Their conclusions were consistent with essentially all galaxies being in groups or clusters, with a continuous distribution in membership from a few to a few thousand. They also found evidence for residual correlations on still larger scales, which they described as possible second-order clustering.

At this higher-order level, binaries were once again first, Shapley (1933) having reported a close cluster pair.

THE EXISTENCE OF SUPERCLUSTERING AND BEYOND

Meanwhile, the 48-inch Schmidt telescope had come into operation in about 1948, with a field of view as large as those of the Lick 36-inches and a much fainter limiting magnitude. Most of the 4000-plus plates (for which the plate holder weighed just over 40 pounds) were taken by George Abell and Albert Wilson (both of whom later said that the process had destroyed their first marriages). This was the Palomar Observatory Sky Survey (POSS).

Wilson (1964, 1969) then drifted off into industry and some rather peculiar cosmological speculations, while Abell stayed on station and compiled an extensive catalog of rich clusters (Abell 1958). Zwicky, et al. (1961-68) independently examined the POSS plates and compiled a six-volume catalog of galaxies and clusters. The Zwicky et al. definition of a cluster was very different from Abell’s, and thereby hangs much of the ensuing squabble. Zwicky’s idea, both then, earlier, and later (e.g. Zwicky 1938, 1963) was that the universe was completely divided into cluster cells, each inhabited by precisely one cluster, of characteristic size 20-40 Mpc (Zwicky and Rudnicki 1963). Zwicky used his cell size to estimate the mass of the graviton as $10^{16} M_{\odot}$. A Zwicky cluster could have considerable substructure within that cell volume.

In other words, Zwicky’s clusters were not so very different from the superclusters that Abell (1961) was advocating – and determining masses near 10^{16}M. for – by the time of the Berkeley IAU and associated symposia. Incidentally, in case you might be feeling that Zwicky took unfair advantage of the hard work of Abell and Wilson, remember that he had been the first to advocate the use of Schmidt telescopes for serious professional astronomy and had worked hard to bring both the 18-inch and 48-inch to Palomar Mountain. He and Wilson remained lifelong friends (through the second marriage of each).

It is, of course, possible to pick out tracers that are not obviously superclustered or even clustered, for instance the 3C and Parkes bright radio sources (Holden 1966, Payne 1967, with a few hundred radio galaxies and quasars in each catalog). For that matter, the 31,000 brightest northern 6-cm sources still look remarkably smooth on the sky to the eye or the Michell test. Nevertheless, it seems a bit odd in retrospect that de Vaucouleurs (1956, 1958, 1970 and many other places) thought himself a lone warrior in the fight for superclustering. He could easily have declared victory in the mid 1960s and gone on to other things.

Instead, de Vaucouleurs (1970 e.g.), who, like McVittie, habitually included a cosmological constant in his mental map of the universe, became a strong advocate of still larger, hierarchical or fractal structure, in the tradition of Swedenborg, Wright, Lambert, and Charlier. He started the argument by drawing an analogy with estimates for the age of the universe. His graph showed 6000 years at the time of Newton and Archbishop Ussher, 30 million in the mid 19th century (the Kelvin-Helmholtz timescale, so called because it was discovered by Meyer and Waterston), and closely spaced, rising points from the 2 Gyr associated with Hubble's value of H to the l0-20 Gyr associated with his and Sandage's H's. The historical data cannot be gainsaid. Indeed a few years earlier, a bunch of us Caltech grad students had plotted H vs. time and concluded that it would go negative in about 1970 and the universe start to recontract. This did not happen. Instead, H has leveled off in a band that was 50-100 in 1970 and perhaps 55-80 today. De Vaucouleurs, however, asked rhetorically, "Would it not be remarkable if our knowledge of the time scale of the universe, after this long period of growth, should suddenly come to a halt just as I am writing this paper?" Remarkable, perhaps, but true,

In a similar way, he plotted the sizes and densities of the largest structures known, pointing out that size had gone up and density down systematically over the century with the recognition of galaxies, clusters, and superclusters. Again he asked, "Would it not be remarkable...?" Again perhaps remarkable, but true. The volumes of the universe surveyed have expanded enormously since 1970, most recently with the 2dF and SDSS results presented at this meeting. But the largest structures seen are no longer the sizes of the survey volumes (the fact which aroused much of de Vaucouleurs's suspicion). Instead, observers now find the same sorts of clusters, sheets, filaments, voids, bubbles, and sponges over and over again.

The largest structures in the universe have, finally, been identified, and we can concentrate, as this conference did, on figuring out how they arose. Indeed one speaker specifically advised that, "If you have a graduate student who wants to work on fractal universes, don't let him." So also wrote Field (1975, but written in about 1970). Nevertheless, you can't quite go home yet. We still have about 1.5 more issues to examine.

LARGE SCALE STRUCTURE IN VELOCITY SPACE

All those lumps of matter, light and dark. are going to pull on each other. The most obvious manifestation is the large velocity dispersion of clusters (Zwicky 1933), but one might also expect net relative motions on larger scales. So said Gamow (1946), and I do not know that he was first. He suggested that, since everything from moons to the Milky Way is rotating, so might be the universe as a whole, or large regions of it.

The master's thesis of Vera Cooper Rubin (1951, 1954) looked for such rotation and other possible systematic motions in a set of about 100 published redshifts. Now, if you live on the outskirts of some structure of poorly known size and location then, with redshift data alone, it is not very easy to distinguish between rotation around some point in a given direction and net motion toward and away from the directions 90° away. Rubin's (1951) analysis suggested both possibilities. The eye of 2002 naturally slides over the "rotation" numbers and focuses on the "expansion/contraction" numbers between 180 and 370 km/sec in roughly the direction of the north polar cap. Oh, one says, she saw what we now call Virgocentric infall, that is, the fact that the expansion of the universe looks slower in the general direction of the center of the supercluster to which we belong.

The first, and for some years almost the only, citations of the work came from de Vaucouleurs (1953, 1958, 1959/60), who invoked the evidence for rotation or retarded expansion or both as support for the existence of his supercluster. His later papers (de Vaucouleurs 1958, 1959/60, 1964) say that the same asymmetry, most likely to be interpreted as inhibited expansion, is to be seen in the data of Humason, Mayall, and Sandage (1956) a classic compilation, whose appendix dropped the Hubble constant further from about 250 to 180 km/sec/Mpc). Not all authors signed all subsections of the paper, and de Vaucouleurs carefully distinguishes the redshifts of Humason and Mayall from the interpretations of Sandage (who later became one of the great, all-time measurers of redshifts). His last sally is the remark that such anisotropy means that the velocities determined from galaxies in the direction of Virgo will be too small by 30% or so and the Hubble constant therefore also too small (and he will fight that battle for the rest of his life, holding on to H = 90-100 to the end).

Close the curtain for about 20 years and bring it up again on a now fully established Vera C. Rubin and her colleagues associated with the Department of Terrestrial Magnetism. She and they embarked on what they described as an unconventional investigation, designed not to measure H or even q but to look for deviations from isotropy of the Hubble law. The first hint of a "yes" answer appears in Rubin et al. (1973). The full-blown result was presented at the 1975 Bloomington AAS meeting and promptly dubbed "the Rubin-Ford effect." This means that you aren't exactly questioning the accuracy of the data, but you don't much like the implications and don't want to be identified with them. The data appear in Rubin et al. (1976).

The problem, as in all such studies, is that, while redshifts require only a large telescope, good detectors (Rubin was one of the first converts to image intensifiers), and dogged determination, distances require ingenuity or divine inspiration. They chose to look only at Sc I galaxies and assume that all were the same real brightness (or rather would be if corrected to face-on), leading to individual luminosity distances for the galaxies and so to velocity residuals relative to smooth Hubble expansion. And yes, there was systematic variation across the sky, and yes, the error bars are large. Their examination of E and S0 velocities reported about the same time by Sandage found residuals that varied across the sky in much the same way. Both implied that the contents of a largish volume of space (including the Local Group) were moving at about 500 km/sec relative to the general expansion.

Another decade passes, and, in January 1986, 68 astronomers gather in Hawaii to discuss Galaxy Distances and Deviations from Universal Expansion (Madore and Tully 1986. According to the editors, "scarcely a single active worker in the field of the distance scale missed the event." Both Sandage and de Vaucouleurs were there and stand adjacent, though not obviously together, in the front row of the conference photo. De Vaucouleurs spoke on H (which was, of course, large), not on superclustering. Sandage is not represented in the proceedings. Nor, more strangely, is Tammann, who was also a participant. Neither Rubin nor Ford of "the effect" participated (and only one or two papers cited them). And Abell was by then dead. But the proceedings include (and the participants heard) two key first reports. One was from the group later called the Seven Samurai, who collected redshifts and distance estimates for elliptical galaxies and searched for deviations from Hubble flow. Their result was consistent, to within the rather large error bars, with that of Rubin et al, (1976) for both amplitude and direction of the large scale peculiar motion we share. Alphabetically (which was not perhaps the actual order of command) they are D. Burstein, R. Davies, A. Dressler, S. Faber, D. Lynden-Bell, R. Terlevich, and G. Wegner.

Also reported in Kona were the first convincing detections of a dipole anisotropy in the 3K background radiation, found almost simultaneously by groups at Princeton (the best-known name was D. Wilkinson) and UC Berkeley (P. Lubin). Their detectors flew on balloons. The amplitude (about 600 km/sec) and direction (galactic longitude = 272 $\pm$ 5 , galactic latitude = 30 $\pm$ 5°) for the motion of the Local Group are described by the authors (Lubin and Villela, in Madore and Tully 1986) as completely consistent with the Rubin et al. (1976) numbers, though later discussions have been less confident about this point. The CMB numbers represent the motion of the Local Group relative to the surface of last scattering, at a redshift near 1000. Lubin and Villela (p 169) quote a "private communication" number from Sandage for the motion of the sun in the Local Group.

TIDYING UP LOOSE ENDS

Can we now say that all astronomers now take large scale structure and streaming seriously, as real phenomena that models of galaxy formation must retrodict or hindcast? Perhaps. Maxwell, Pauli, and others have been credited with remarks along the general lines that "everyone now believes in the wave theory of light, because all the people who believed in the particle theory of light are dead." Hidden under the carpet throughout the last section or two has been a small group of astronomers who have persisted in attributing apparently large scale structure on the sky (in both two and three dimensions) to differential obscuration in the Milky Way. The suggestion appears in a paper by Viktor A. Ambartsumian (1940), who eventually focussed on a different idea, that large scale clusters existed but were not gravitationally bound and somehow represented matter newly appearing or newly changing state in the universe. Tadeos Agekyan (1957), a student of Ambartumyan, carried the differential absorption point of view to Leningrad/St. Petersburg, where he passed it on to B. I. Fasenko. Fasenko (1996) carried the banner for a number of years, but has not published (on that or anything else) since 1996 in journals that are covered by A&A Abstracts. Agekyan still appears in the IAU directory at St. Petersburg University (Fasenko was never a member) but also has not published on this subject in a number of years.

Thus it seems that all the people who favored differential obscuration over very large scale structure are, if not dead, anyhow no longer on the front lines. This is, of course, not quite the same as saying that we have hold of all parts of the right answer. Conference participants perhaps gradually became aware that all the overheads that had words near the top had reproductions of well-known optical illusions and impossible objects (about half from Escher) at the bottom, ending with the profile of a fairly scholarly-looking chap, drawn by Paul Agule, who rotated 90 ° to the hand-written word "liar."

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